to suitable axes) is everywhere zero—at least so far as electromagnetic phenomena are concerned.

Though I find myself in agreement with Prof. Richardson's conclusion that magnetic intensity is not to be identified with speed of æthereal flow, as explained in his letter to NATURE of May 23, I venture to dissent from his arguments. These proceed from the contention that, on the contested assumption, certain integrals would become infinite. Now, in the first place, it appears to me from mere inspection that both these integrals (which I have not actually evaluated) are in reality finite; in the second place, neither integral expresses a magnitude which bears directly on the point at issue, one of them being justly criticised by Sir Oliver Lodge in NATURE of June 6 as apparently devoid of mathematical meaning. The question proposed is as to the momentum due to an electric charge upon a moving sphere, and in this connection the really significant magnitude is the kinetic energy, expressed in terms of the translational velocity. Differentiating this expression with respect to the velocity, we have at once the momentum, the result obtained being independent of any physical theory as to the ultimate nature of the energy in a magnetic field.   C. V. BURTON.

Cambridge, June 8.

Decomposition of Radium Bromide.

YESTERDAY, on opening a glass tube containing 1 milligram of radium bromide which had been hermetically sealed for almost exactly twelve months, there was a very strong odour of bromine which hung about the tube for about ten minutes. The amount of the bromide decomposed in this period would be about 5.4×10^{-7} grams according to Rutherford; the amount of bromine corresponding to this would be about 2×10^{-7} grams. Perhaps some chemist could say definitely whether this amount of bromine would be detectable by its odour. The volume of the tube was about 4 cubic centimetres.

ALFRED W. PORTER.

University College, London, June 8.

The Mass of the α Particle.

APPARENTLY the following simple and obvious method of calculating the mass of the α particle has been overlooked.

According to Rutherford, the number of α particles emitted per second by a gram of pure radium is 2.5×10^{11}. Of these particles, one-quarter comes from each of the four elements Ra, RaEm, RaA, RaC. The particles from these four elements are emitted with velocities $0.82 \cdot V_0$, $0.87 V_0$, $0.90 V_0$, $1.00 V_0$ respectively, where V_0 is 2.6×10^9 cm./sec.; they all cease to produce ionisation when their velocity is $0.43 V_0$. Hence the loss of kinetic energy of all the α particles emitted from one gram of radium in passing over their ionising ranges is

$$\frac{2.5 \times 10^{11}}{4} \times \tfrac{1}{2}m \times \{(0.82)^2 + (0.87)^2 + (0.90)^2 + 1^2 - 4 \times$$
$$(0.43)^2\}(2.6 \times 10^9)^2 = m \times 5.3 \times 10^{29},$$

where m is the mass of an α particle.

At the same time, it is known that one gram of radium gives out 105 gram-calories per hour (mean value), or 1.22×10^6 ergs per second. If we may identify this quantity of heat energy with the kinetic energy lost by the α particles in ionisation we have

$$m \times 5.3 \times 10^{29} = 1.22 \times 10^6$$

or

$$m = 2.3 \times 10^{-24}.$$

The ratio e/m for the α particle is 1.56×10^{14} electrostatic units. The two most probable theories of the nature of the α particle are (1) that it consists of an atom of helium carrying a charge 2ϵ, where ϵ is the electronic charge 3.4×10^{-10}, and (2) that it is a molecule of hydrogen carrying a charge ϵ. On the hypothesis (1) the mass of the particle is 4.26×10^{-24}; on the hypothesis (2) it is 2.13×10^{-24}. The calculation given indicates that (2) is correct, and explains the failure of Greinacher and Kernbaum to obtain helium from the α rays of polonium (*Phys. Zeit.*, 1907, p. 339).

If it be assumed that the whole of the kinetic energy of the α particles, and not only that part of it which is spent in ionisation, appears as heat energy, the value for m is found to be

$$1.78 \times 10^{-24}.$$

I have thought it best to give the maximum estimate of that quantity which can be attained by this method.

NORMAN R. CAMPBELL.

Trinity College, Cambridge, June 3.

The "Renal-portal System" and Kidney Secretion.

I RECENTLY published a short paper (Proc. Zool. Soc., 1906) on the significance of the so-called "renal-portal system" found in most of the lower Vertebrata. In this paper I advanced strong reasons for supposing that the "renal-portal system," or, as I prefer to call it, renal cardinal meshwork, is non-excretory in nature. I showed that, both developmentally and structurally, there was every reason to doubt whether the renal cardinal meshwork takes any part in the formation of the plexus of blood-vessels which surrounds the urinary tubules (although, of course, these are connected with each other), and that therefore the blood apparently supplied to the kidney by the "renal-portal" (post-renal) vein is in all probability not utilised in the production of the kidney secretion. This conclusion, opposed to that held by most physiologists and morphologists, I supported by citing the physiological experiments of Nussbaum (*Pfluger's Archiv*, xvi., xvii., 1878; *Anat. Anzeig.*, i., 1886) and Beddard (*Jour. Physiol.*, xxviii., 1902), which afforded valuable confirmation. These experiments, as is well known, proved that after the arterial supply of the frog's kidney had been eliminated all secretion immediately stopped, notwithstanding the facts that the "renal-portal" circulation was still in full swing and that powerful diuretics were employed. The sole objection to regarding these experiments as conclusive was that, in consequence of the kidney being deprived of oxygenated blood, the tubular epithelium had degenerated, and was therefore not in a condition to secrete. While recognising this objection, yet for the other reasons which I had already advanced I ventured to maintain that, even if the blood in the post-renal vein could be artificially oxygenated, no secretion would occur.

Unfortunately, I was not aware of more recent physiological work on this subject when I made this last suggestion. Since then, however, Prof. Halliburton has kindly directed my attention to the papers of Bainbridge and Beddard (*Biochemical Journal*, i., 1906) and Cullis (*Jour. Physiol.*, xxxiv., 1906), in which the reverse result has been obtained; that is to say, according to these later experiments, a secretion can be obtained from the "renal-portal" circulation provided that the tubule epithelium is maintained in a healthy condition by means of a sufficient supply of oxygen, and that powerful diuretics like urea and phloridzin are employed. This result at first sight appears to be contradictory of my previous conclusion and confirmatory of the generally accepted "portal" theory of the renal cardinal meshwork, but it is the object of these remarks to show that such is, after all, not necessarily the case.

In the first place, these recent experiments have clearly shown that the "renal-portal" circulation will not yield the slightest secretion in the absence of powerful diuretics; in other words, the result obtained by Bainbridge, Beddard, and Cullis is at best an abnormal one. Under more normal conditions, *i.e.* in the absence of diuretics, with a healthy tubule epithelium and with the "renal-portal system" alone working, no secretion whatever occurs.

Secondly, the very fact that when the venous blood contained in the renal cardinal meshwork alone "supplies" the kidney, the tubule epithelium degenerates, proves that in the normal living animal this blood is not in contact with the tubules, *i.e.* does not take part in the formation of the blood-plexus surrounding the tubules, since, as the experiments prove, these latter require the oxygenated blood derived from the renal arteries in order to live and much more to secrete.

Thirdly, it must be remembered that in the experiments of Bainbridge, Beddard, and Cullis, the elimination of the

arterial circulation of the kidney does away with the blood current which normally, flows *away from* the region of the tubules, and, this being the case, the venous blood of the renal cardinal meshwork, encountering no resistance, is enabled to penetrate to the tubule plexus, carrying with it the injected diuretics which cause the secretion observed. There is no reason why the secretion should not occur under these abnormal conditions. The tubule epithelium is well supplied with oxygen, the veins are gorged with impure blood, and in experiments in which at all large secretions were obtained the pressure was artificially raised by forced injection or otherwise.

It is easy, then, to account for the secretion obtained by the investigators named, and at the same time to believe that the venous blood takes no share in the formation of the kidney secretion under normal conditions.

It has always surprised me, speaking as an outsider, that physiologists have so readily assumed that they possess in the frog and other animals with " portal " kidneys so many convenient anatomical contrivances in which the glomeruli and the renal tubules are supplied by separate vessels. It is true that the renal cardinal meshwork is continuous with the blood plexus surrounding the tubules, but surely it is very unsafe to assume from this one fact that the venous blood is used by the tubules for secretory purposes. Another equally patent fact, that the similar tubules of mammals employ arterial blood, should suffice to cast doubt on the assumption. And when we recall to mind the statements of Hyrtl (*Wiener Akad. SB.*, xlvii., 1863) and Vialleton (*C. R. Hebdom. Séances Soc. Biol. Paris*, liv., 1902), among others, that in those " portal " kidneys in which the vascularisation has been histologically examined, viz. those of the frog and certain sharks, the renal cardinal meshwork is structurally distinguishable from the tubule plexus (the former consisting essentially of large channels putting the post-renal vein into communication with the post-caval, and the latter consisting of capillaries which open into the channels), there is still more reason for supposing that the flow of blood is from the tubule plexus into the renal cardinal meshwork, and not in the contrary direction. The numerous experiments which have been based on the aforesaid assumption have, I should imagine, given rise to incorrect ideas as to the normal functions of the urinary tubules.

If I needed any additional physiological evidence in support of my contention that the post-renal vein has nothing to do with the vascular plexus of the urinary tubule, *i.e.* does not supply the kidney for excretory purposes, I find it to hand in the recently issued British Association Report for 1906, York. In a report on " The ' Metabolic Balance Sheet ' of the Individual Tissues," p. 427, it is shown to be exceedingly probable, by the relative amounts of oxygen used up by the kidney tissue of a frog and a mammal respectively, that the " renal-portal " vein of the frog bears a very different relation to the kidney tubules as compared with that of the renal vein of mammals—which is the conclusion I am maintaining. It is further stated that " when the same kidney is perfused at different times through the aorta and through the renal-portal system, there is a greater consumption of oxygen in the former case than in the latter (double to treble in four experiments)." If we assume what is generally held to be a well-established fact, viz. that the kidney-tubule epithelium plays quite as important a part in kidney secretion as the glomerular epithelium, then it is difficult to understand, on the portal theory of the kidney, why the quantity of oxygen absorbed by the kidney tubule is totally out of proportion to the work done by it. Obviously the only rational conclusion to draw is that in the above experiment the oxygen perfused through the " renal-portal " vein did not come into contact with the tubule.

To sum up, I think I may say that I have clearly shown that the recent work of Bainbridge, Beddard, and Cullis does not disprove my original contention that the renal cardinal meshwork is, under natural conditions, non-excretory, that, in short, the so-called " renal-portal " vein does not supply the renal tubules, as physiologists commonly assume, and that, in consequence, experiments based on this assumption are liable to give rise to misleading ideas. W. WOODLAND.

Mendelism and Biometry.

IN the striking and suggestive review of Mr. Punnett's work on " Mendelism," in NATURE of May 23, the reviewer cites, without naming the author, a view expressed by Mr. A. D. Darbishire (*Manchester Memoirs*, 1906) to the effect that " the Mendelian deals with units and the biometrician with masses," and states that this idea, " though plausible, is based on a fallacy," for " the Mendelian's units *are* the biometrician's masses, except when the latter exceeds his limits and includes within his masses more than one such unit."

I have no doubt that Mr. Darbishire read the review with as much enjoyment as myself, but it seems to me that his statement of the case is dismissed with scarcely sufficient consideration. The reviewer's points, if I understand aright, are two :—(1) that Mendel's laws (by which he seems to mean, not merely the law of segregation, but the laws of observed proportions) are really mass-laws and not laws of the individual; (2) that the biometrician's masses are the masses to which Mendel's laws apply. But surely (1) Mendel's laws are based on definite conceptions as to the germ-cells of the *individual*—and that is the important point—and are true of the *individual* to a degree of approximation which is the higher the greater the number of offspring (quite a high degree in such a case as Mr. Lock's maize). Further, (2) if the " Mendelian's units " were the " biometrician's masses," there should be inheritance of individual variations, within each of two races A and *a*, for any character to which Mendel's laws applied on crossing those races; for inheritance of individual variations is what the biometrician has observed for nearly all characters in his masses.

I indicated the importance of an investigation on this point some time ago (*New Phytologist*, i., 234)—for it is almost a fundamental question whether a single determinant, such as may be assumed to exist for a unit Mendelian character, is or is not capable of variation from individual to individual—but I am not aware that any such investigation has been made. The reviewer's assumption may, therefore, be true, but it is unproven, and theories at present in the field (Pearson, *Phil. Trans.*, 1904; Yule, *Conference on Genetics*, 1906) are based on the opposite assumption, viz. that heritable individual variations are due to the character concerned being determined by *n* allelomorphic couplets, and not by one. If this be true, the " biometrician's masses " are precisely masses to which Mendel's laws, in their simplest form, do *not* apply.

The question referred to above, whether a unit Mendelian character exhibits heritable individual variations or no, seems to be one that urgently calls for experimental investigation. G. UDNY YULE.

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MR. YULE is probably right. The question is this : Is the inheritance which the biometrician always finds within the limits of his masses due to the fact that he is dealing with a large number of Mendelian units, or that he is measuring the intensity of heredity *within* such a unit?

If the former, Mr. Yule is right in saying that I criticised the view expressed by Mr. Darbishire unjustly. If the latter, the mass of the biometrician *is* the unit of the Mendelian. But before we can give an answer to this question we must know, as Mr. Yule points out, whether there is inheritance of fluctuating variations within the limits of a single Mendelian character such as " tall," in peas. If we may argue from stature in man to stature in peas, we should compare the character tall (or normal) in peas to tall (or normal) in man, and dwarf in peas to dwarf in man. We know that there is inheritance within the character tall in man, and, if this analogy is legitimate, we should expect to find it so in peas. If it were, the answer to the question whether the view expressed by Mr. Darbishire were right or not would depend on whether we still called the character tall the unit or extended the conception of unit to the smallest heritable variation within the category tall.

THE REVIEWER.